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Interactive comment on "A new classification of satellite derived liquid water cloud regimes at cloud scale" *by* Claudia Unglaub et al.

Anonymous Referee #2

Received and published: 21 November 2019

The paper describes the use of CloudSat and CALIPSO retrievals of cloud base and cloud top heights to classify low (water) clouds in the atmosphere across the globe. The data are then combined with MODIS retrievals of number concentration and liquid water path to show the utility of the classification in distinguishing between different cloud states. The study is nice addition to the body of work on cloud classification from satellite observations. The paper is well-written and follows a logical flow of arguments. Its weaknesses lie in an incomplete description of the techniques applied as well as a lack of discussion of some obvious caveats in the results. I expand on both in my comments below. These issues should be straightforward to address and the paper will be acceptable for publication once they are.

Major comments

[Printer-friendly version]

[Discussion paper]

Description of the techniques used:

Lines 62-65: The paragraph introduces the CCCM data set that forms the foundation of this study. For the reader to fully comprehend how the classification works and how it might later link to the MODIS retrievals it is necessary to add a short paragraph here that provides a little more detail on how this merged product was created. How is the vertically resolved but horizontally sparse CloudSat and CALIPSO data combined with horizontally resolved MODIS data? What does it mean to use the CloudSat and CALIPSO data on a 20 km footprint? I realise there is a set of papers to read up on this, but not only would it help attract a greater readership if a short summary was provided here, it is also essential to understand the results that follow.

Line 117: You mention the use of adjacent footprints to look at the variability. What does adjacent mean here? Adjacent in which direction? I assume it is adjacent along the Aqua track? Please provide a little more detail here.

Discussion of caveats

The biggest caveat of the study is that the choice of looking at liquid water clouds only automatically introduces a geographic distribution to the result. This is so, because the 0C level in the tropics is at 5 (or so) km and in the polar regions it's very low (or even non-existent). This will bias liquid water path to be larger in the tropics and smaller in the extratropics. As the cloud classes show a strong geographic distribution (Figure 3), the separation of liquid water path by cloud class in Figure 4 could simply be a geographic sampling issue and have nothing to do with cloud-aerosol interactions. There is probably little you can really do about this at the definition stage, but you can account for it in the analysis of N_d and L and you MUST discuss that this as an issue in your conclusions.

I recommend to reduce this issue by performing analyses of the N_d and L pdfs in selected regions. As a minimum one might want to separate the (warm) tropics from the (cold) extra tropics. You could then contrast say land and ocean in both regions

and see if something useful emerges. That way you can alleviate at least some of the concern that the L differences are just difference in physical thickness resulting from the fact that different cloud classes preferably occur in the tropics (e.g., inhomogeneous C_B,mid) or the mid-latitudes (C_B,low).

Places where this fits in the manuscript are near line 165 or 175 and in the summary and conclusions.

Minor comments (on chronological order)

Line 29: Parentheses missing around the citations.

Line 96: Why do you use "roughly terciles" rather than terciles? How rough is rough? What are the tercile values compared to what you use? Presumably you just want round numbers or is there more to it?

Line 101: Add 'and' between homogeneous and cumuliform

Line 121: Actually, the shallow Cu regions show remarkably little variance compared to the deep tropics! As you are only using water clouds and therefore exclude deep convection, this is a little surprising. Is this a physical signal or is there something difficult about your method in the deep tropics that might cause this result? Please discuss this more deeply.

Line 124: The statement that your classification can separate stratiform and cumuliform is true to first order, but the trade regime, which is mostly cumuliform, does not show up very strongly. This makes sense as the inversions there are still quite strong and even though the clouds might be cumuliform, their tops are tightly constrained by the inversion height. So there is a caveat on your simple inhomogeneity assumption here, that you should acknowledge.

Line 130: Delete: 'Consistent with the expectation', as this certainly was not my expectation and might not be that of all readers.

Figure 3: Bottom right panel is mislabeled.

Line 136: The low H-base clouds nicely follow regions of low SST. It might be worth mentioning this here.

Line 139 and through the paper: I believe it is dangerous to refer to you classes as "mid-level clouds" or "high clouds", as they are not. Please be diligent in using terms like low-base, mid-level-base or high-base clouds. You do this in parts already and it is important to stick to that to minimise confusion with real mid-level or high clouds in the atmosphere. Please go carefully through the entire manuscript to fix this everywhere.

Summary and conclusions section: Please add a short discussion as what you perceive to be the weaknesses of your methodology and how those could be addressed in future work.